breast and under surface purple; back and wings rich chestnut, with violet reflections when viewed in certain lights, and passing into golden bronze at the nape; rump and upper tail-coverts rich purplish blue; tail blackish green; legs yellow or reddish yellow.

Total length 16 inches; bill $1\frac{1}{4}$, wing 7, tail $6\frac{3}{4}$, tarsi $2\frac{3}{4}$.

I obtained this fine bird of Mr. James Gardner of Holborn, who could not inform me of the precise locality in which it was collected; but as it was accompanied by *Paradisea papuana*, *Epimachus maximus*, many specimens of *Semioptera Wallacei*, and *Pitta maxima*, it was probably procured on some one of the islands of the Eastern Archipelago or in New Guinea. Although the bill is not toothed, this species appears to be allied to *Didunculus*.

MISCELLANEOUS.

Deep-sea Researches.

To the Editors of the Annals and Magazine of Natural History.

Gentlemen,—You will oblige me, and at the same time, I believe, further the interests of scientific truth, by inserting the following observations in the 'Annals.'

In a note which appeared in 'Nature,' of Dec. 16th, p. 192, Mr. Gwyn Jeffreys makes known his views on the "*Food of Oceanic Animals*" in these words:—

"The receipt of an interesting paper by Prof. Dickie, entitled "Notes on range in depth of marine Algæ," lately published by the Botanical Society of Edinburgh, induces me to call the attention of physiologists to the fact that plant-life appears to be absent in the ocean, with the exception of a comparatively narrow fringe (known as the littoral and laminarian zones) which girds the coasts, and of the "Sargasso" tract in the Gulf of Mexico.

"During the recent exploration, in H.M.S. 'Porcupine,' of part of the North Atlantic, *I could not detect the slightest trace of any vegetable organism at a greater depth than fifteen fathoms. Animal organisms of all kinds and sizes, living and dead, were everywhere abundant, from the surface to the bottom*; and it might at first be supposed that such constituted the only food of the oceanic animals which were observed, some of them being zoophagons, others sarcophagons, none phytophagons. But inasmuch as all animals are said to exhale carbonic acid gas, and on their death the same gas is given out by their decomposition, whence do oceanic animals get that supply of carbon which terrestrial and littoral or shallow-water animals derive, directly or indirectly, from plants? Can any class of marine animals assimilate the carbon contained in the sea, as plants assimilate the carbon contained in the air?

"Not being a physiologist, I will not presume to offer an opinion: but the suggestions or questions which I have ventured to submit may perhaps be worth consideration. At all events the usual theory, that all animals ultimately depend for their nourishment on vegetable life, seems not to be applicable to the main ocean, and consequently not to one-half of the earth's surface.

"J. Gwyn Jeffreys."

It is quite unnecessary for me to criticise the remarkable opinions here offered regarding the Sargasso tract, the chemistry of decomposing animal matter in the ocean, and the relative proportions of land and water on the globe. It will be seen that they are unique. But as Mr. Jeffreys has entered the lists as an authority on deep-sea lore, and now claims as his own the discovery that plant-life is absent in the deeper regions of the ocean, and the refutation of the theory (as applied to the inhabitants of the sea) that "all animals ultimately depend for their nourishment on vegetable life," I must be excused if I endeavour to show that he has either forgotten what, at a not very remote period, he professed to have read of my writings on these subjects, or that, not having forgotten them, he has nevertheless found it expedient, for some unaccountable reason, to repudiate them, and with them his own published estimate regarding their accuracy.

The absence of all living vegetation, even of the lowest types, in the deeper abysses of the ocean, and the vital process whereby the nutrition of the lowest animal forms is secured in failure of anything like a rudimentary digestive apparatus, such as is to be found in the higher orders of Rhizopods, was dwelt on by me in my 'Notes on the presence of Animal Life at great depths in the Ocean,' published in Nov. 1860, p. 27,—in my 'North-Atlantic Sea-bed,' published in 1862, pp. 130–132,—in a note which appeared in the 'Annals & Mag. Nat. Hist.' for Aug. 1863, p. 166,—and in two papers contributed by me to the 'Monthly Journal of Microscopical Science,' for Jan. 1869, pp. 39–40, and April of the same year, pp. 231–233. Reference to these publications will therefore show that Mr. Jeffreys's statements are, to say the least of them, somewhat behind the times.

But to prove that Mr. Jeffreys cannot justly plead ignorance as to what had been previously published by me on the subject, I invite attention to two extracts from his "Reports on Dredgings," contained in the 'Annals' of the respective dates given below.

'Annals,' Nov. 1866, p. 387.	'Annals,' Oct. 1868, p. 305.
"Dr. Wallich, in his admirable and "philosophical treatise with which all "marine zoologists and geologists are, "or ought to be, familiar, believed that "certain starfishes" &c. &c. "*As to "the accuracy of his statements, no reasonable doubt can be entertained.*"	"Coccospheres and free Foraminifera cover the bed of the Atlantic at "enormous depths. The occurrence, "therefore, of such organisms on the "floor of the ocean at great depths "does not prove that they ever lived "there. *I should rather be inclined to "believe that they dropped to the "bottom when dead or after having "passed through the stomachs of other "animals which had fed on them*"

It is indisputable, therefore, that Mr. Jeffreys had studied my writings, and that the opinion entertained of them by him in 1866 was revoked in favour of that expressed by him in 1868; whilst that expressed in 1868 has again in its turn been superseded by the very positive contradiction it receives in his note in 'Nature' published a fortnight ago!

It is likewise deserving of special notice that Dr. Carpenter, who might be supposed to have made himself acquainted with the whole past literature of the subject, should, at p. 181 of the official copy of his 'Preliminary Report on Dredging for 1868,' have thought it expedient to single out from these two most conflicting statements that which was offered by Mr. Jeffreys in 1868 (see above), as evidence that "*Dr. Wallich's just claims had not by any means commanded the universal assent of naturalists*"—an assent to which, if just, as it has now been most clearly proved that they were and are, those conclusions were long ago entitled.

With regard to Mr. Jeffreys's division of oceanic animals into "zoophagons" and "sarcophagons," I have nothing to urge beyond my avowed inability to discern any valid physiological difference between those that are zoophagous and those that are sarcophagous. It rests with Mr. Jeffreys to explain on what grounds he has felt justified in declaring so emphatically that "*none*" of the animals "*of all kinds and sizes, everywhere abundant from the surface to the bottom*," observed by him in his exploration of the North Atlantic, were phytophagous.

It only remains for me to add that for years I stood alone in maintaining, in opposition to the opinion of Ehrenberg and his followers, that all plant-life becomes extinct at depths exceeding 400 or 500 fathoms, and that the nutrition of the Foraminifera and some other orders of oceanic Rhizopods is effected by a special vital function, whereby they are enabled to eliminate, from the medium in which they live, the elementary ingredients which enter into the formation of their body- and shell-substances. The facts and reasoning on which my observations were based will be found in the various published papers &c. already referred to.

I remain, Gentlemen,
Yours very faithfully,
G. C. Wallich.

Kensington,
December 24, 1869.

On the Specific Distinctness of Anodonta anatina.

To the Editors of the Annals and Magazine of Natural History.

Gentlemen,—There has been, and, I believe, still is, a diversity of opinion as to whether *Anodonta anatina* is a distinct species or only a variety of *Anodonta cygnea*. I have, since I commenced the study of conchology, inclined to the former view; and I think I am now able to bring forward evidence in favour of it which has not been